

San Joaquin River Fall-run Chinook Salmon Population Model
External Scientific Review Form

Reviewer: #3

Review:

This is such an important issue, especially with the pending effort to restore salmon on the mainstem San Joaquin River, that I have expanded much more on a number of points than I would normally. I believe that a useful salmon population modeling tool is ripe for development on the SJ but this is not that tool. The authors appear to be using Vernalis flow in much the same way that X2 was used as an ecosystem indicator but there are three significant differences: 1) we know a lot of the mechanisms affecting salmon dynamics which we still don't know for the X2 species, 2) the X2 effort was collaboration of diverse scientists working together in the context of clear connections to relevant ecological literature, and 3) the correlations between X2 and species abundance were not driven by a couple of extreme data points.

1. **Problem/Goals.** Is the problem that the project is designed to address adequately described? Are the goals, objectives and hypotheses clearly stated and internally consistent?

The author identifies four questions

- 1) the status of SJR salmon populations
- 2) what level of protection is being afforded salmon smolts outmigrating from the SJR into south delta?
- 3) what is the status of the VAMP?
- 4) What influence does spring flow have on SJR salmon production?

Question 1 is quickly answered; SJ salmon are not doing well.

Question 2 is the heart of the SWRCB request that led to this model. By referring to the adequacy of VAMP it is clear that the question is about survival of smolts through the delta.

Question 3 identifies that VAMP has not addressed conditions of 7000 cfs and that it has therefore not yet reduced the uncertainty surrounding the effects of flow and exports on survival of smolts through the south delta.

Question 4 is an extrapolation from Question 2 to cover all the effects of flow in on salmon in the watershed. This, unfortunately, became the goal of this model which now tries to use Vernalis flow as a surrogate for upstream habitat, temperature, transport and all other impacts of flow on salmon.

I am puzzled by the assumption that the SWRCB's request to provide comments on the adequacy of the 1995 Water Quality Control Plan for San Joaquin salmon referred only to the springtime flow requirements. The WQCP not only addresses springtime flows, but also October flow conditions to permit better returns of adults to the San Joaquin River. Are these measures adequate? How have they been implemented? Passing comments are made to the

return of fall run adults and there is a reference to the reduced dissolved oxygen problem near Stockton, but no discussion of adult migration exists in this report. Does the Department feel that adult returns are not an issue or that the 1995 WQCP is fully adequate?

From this discussion of the WQCP inadequacies and concerns, this report clearly attempts to support the model's assumption that flow at Vernalis is the only issue controlling the abundance of San Joaquin salmon. The goal of this report then becomes to conclude that the size and duration of the flows in the WQCP are inadequate and that the recommendations given to the SWRCB by DFG are based on a reasonable interpretation of all available information. I do not comment on the structure of the model, which could be very useful if given the data relevant to delta survival. Instead most of my comments will address the model's choice of variables and input data.

The executive summary concludes with a discussion of the use of hatcheries which seems entirely separate from the SWRCB request for the evaluation of flows. Unlike the treatment of other factors affecting the abundance of salmon, no evidence is presented that hatchery production has affected escapement to the SJR. Amongst the suggested caveats are efforts to protect the genetic integrity of SJR salmon. However, the footnote on page 21 refers to a report that there is no genetic distinction between Sacramento and San Joaquin stocks. The authors of that report conclude that the previous 140 year of hatchery operations in the Central Valley are to blame for the loss of genetic integrity. As with most recent work on the effects of hatcheries, this suggests that greater use of hatcheries is a dangerous tool to use in managing salmon stocks.

2. **Approach.** Is the approach well designed and appropriate for meeting the objectives of the project as described in the proposal? No.

The SWRCB asked only for what flows were needed to protect smolt passage through the delta. The model attempts to use flow at Vernalis as representing two separate things – not only the influences of flow (and the Head of Old River barrier) on smolt passage through the delta but also as a surrogate for all effects of flow on the tributaries. To assess delta survival the model could be used and fitted to survival data either from the CWT studies by FWS and/or with ocean recovery data. Unfortunately, because of this second use, the model has been fitted to overall abundance estimates which are driven by many factors that are related to Vernalis flow but separate from it. In fitting the abundance estimates to flow, the analysis is overwhelmed by two flood years when abundance was high and this results in model results that simply suggest that all years should be flood years.

For just the purpose of assessing flow needs at Vernalis the author correctly states that the data are not adequate to address the issue and flows in the intermediate range (7000 cfs) will be required. The clear recognition of this fact on p 10, where the author suggests that the SWRCB should ensure that such flows occur in the second half of VAMP, seems completely at odds with the description of the construction of the model that relies on the same data that the author describes as inadequate.

3. **Feasibility.** Is the approach fully documented and technically feasible? What is the likelihood of success? Is the scale of the project consistent with the objectives?

The structure of the model is clear. The decisions on which variables to include are much less clear. Many factors are inappropriately left out of the model for its use as an overall population model. Two factors are included without clear evidence that they are significant controllers of escapement: the role of number of days of flow and the relationship between outmigrant smolt abundance and subsequent abundance of adults. As stated above, the attempt to make a model to predict escapement far exceeds the grasp of the available data as well as the request cited from the SWRCB.

4. **Project Performance Evaluation Plan.** Will a monitoring plan be developed to document changes in the restored habitat over time and the response of salmonids and/or riparian vegetation to the restoration in a scientifically rigorous manner?

N/A

5. **Expected Products/Outcomes.** Are products of value likely from the project?

No. The model results provide a linear interpolation between data from flood years and low flow years. Without data from intermediate flow years the outputs of this model cannot be supported.

Additional Questions:

General:

The purpose of the model is to develop spring flow magnitude, duration, and frequency instream flow levels into the South Delta to adequately protect, and restore, fall-run Chinook salmon in the San Joaquin River basin. To accomplish this objective, please address the topics listed below for these questions:

Please note comments above about modeling survival through the delta vs. modeling SJR salmon recruitment. Below I address this model as a model of salmon escapement.

Is the model adequate?

No, not as a tool to manage SJ salmon freshwater life stages. It ignores significant factors, it gives unsupportable reasons for leaving out other factors and it does not address factors affecting fry or adult abundances and it fails to make the case that SJ smolt production controls adult production.

If not, how can model be improved?

There are two paths that this effort could pursue much more successfully:

1. Feed into this model data relevant to the survival of smolts through the delta. These data are already referred to in the report but they are a trivial part of the effort to model the entire population dynamics of SJ salmon by reference to flow at Vernalis. Sadly, as the author explains, the VAMP needs to apply its higher flow levels if we are to have any hope of relevant data about the impacts of flow and exports on success of smolt passage.
2. Develop a biological model, perhaps something like Steve Kramer's winter-run model, to incorporate all the several strands of information that have been developed for SJ salmon. This would not only tie together recent developments in temperature modeling, but allow inclusion of all factors that might affect salmon production and evaluate which ones are important, which ones we need to know more about, and how to integrate things like the in-stream effects of flow on each tributary with other management options like gravel restoration and Vernalis flow.

The model as it currently exists largely can only recommend that we have flood year flows every year. Doubtless that would solve various salmon problems but it is not useful guidance to management or research.

1. Foundation (justification)
2. Logic
3. Numeric representations
4. Application and reliability
5. Conclusions
6. Calibration and validation
7. Documentation
8. Testing (i.e. what monitoring could occur to validate or reject model predictions)

The Justification and logic (1&2) of the model fail to support the assumptions of the model and therefore I have written about each section:

Discussion of Fry importance. p. 10-11.

The author's justification for the focus on smolts rather than fry are inconsistent with my understanding of salmon biology (presumably they mean in regard to conditions in the delta)

1 – 'the fry contribution to escapement is unknown' – ignorance of something does not make it unimportant (the more I read that sentence the more peculiar it becomes).

2 – 'the fact that fry are abundant in the delta in years that are usually productive of adults is negated by the fact that smolts are also abundant in those years.' Clearly since smolts can only come from fry, then an abundance of smolts and fry suggest a high production of young in those years and it is impossible to assess the relative contributions of fry and smolt from the data in hand.

3 – 'Low dissolved oxygen is problematic in the Stockton Deep Water Ship Channel', but fry are abundant in the delta only in wet years when SJR flows are high. The latest report from the people working on the Stockton Deepwater Ship Channel DO issue conclude that SJR flows greater than 2000 cfs negate any DO problems

4 – Smolts from all years return as adults – but their relative roles in regard to fry that reared in the delta is unknown.

5 – ‘There is a strong correlation with smolt production and adult cohort production’ which is simply a repeat of number 2 and still does not account for the fact that years of high abundance of fry in the delta also shows a strong correlation with cohort production.

In other systems it is common that salmon display an assortment of life history patterns (known as ‘phenotypic plasticity’ across years and in different habitats such that they may rely on fry growth in downstream areas in wet years but on fry and smolt growth in upstream areas in drier years. The author’s attempt to homogenize salmon life histories undermines many of the analyses in this report.

The author goes on to recommend the “cessation of late winter/early spring freshets” to reduce the movement of fry downstream. This recommendation is not only counter to the idea of phenotypic plasticity but comes despite the author’s later description of how higher flows likely increase spawning areas in the tributaries. I am unaware of any other salmon management efforts that suggest reducing spawning areas and reducing transport of young downstream.

Flow at Vernalis and its correlates. p 11 &12.

The author describes how flow at Vernalis is tightly correlated with flows from the three tributaries, a result that is singularly unsurprising. The author uses this correlation to justify their attention on Vernalis flows alone, as a surrogate for all other upstream flow effects. This use of Vernalis flows leads to several unfortunate implications. Other methods have been discussed to enhance Vernalis flows that will have no impact on conditions in the tributaries – recirculation of releases from the Delta-Mendota pool and restoration of flows from the San Joaquin. In addition it leaves the possibility of serious discrepancies among the tributary flows; if the Merced is contributing higher flows to Vernalis, that will not improve conditions on the Tuolumne or Merced. A lot of excellent work has gone into temperature modeling of the tributary streams but more remains to be done. This use of Vernalis as a surrogate for upstream conditions suggests that such work is not necessary.

Even on the tributaries, the author’s strong correlations of temperature with flow provides a weak basis for recommending flows. Historical conditions reflect the relationship of reservoir inflow and reservoir releases. If higher releases are mandated at lower reservoir inflows (i.e. when precipitation and the snowpack are smaller than in the historic relationship, the same level of decreased temperature per increase in flow is unlikely to be attained. Such uncertainties are well addressed by the temperature modeling that is going on, but this model does not reflect such interactions.

The flows at Vernalis are shown to correlate well both with the flows on the tributaries and with the San Joaquin River Index that incorporates information on reservoir storage as well as current precipitation patterns. Strong linear relationships are presented tying all of these flow parameters together. However, as they move into developing their model, the author chooses to translate these linear relationships into the standard 5 year-types and thereby obscure the fact that in many years the timing of flows changes considerably so that a below-normal year might have a wet winter but provide very warm, low-flow conditions in spring when smolt are at risk. Retaining the physical connections between reservoir inflows, reservoir releases,

conditions in the tributary streams and their relationship to conditions at Vernalis would provide a model that reflected the known ties amongst various aspects of flow and their impacts on different life stages of salmon, This model does not do that.

Delta exports. p. 14

The author accurately reiterates the history that led to development of the VAMP – all years when salmon production was high occurred when the San Joaquin was in flood with Vernalis flows in excess of 10,000 cfs. All other years production was poor and flows were less than 2,000 cfs. VAMP is an experimental condition that tries to determine what controllable flows in the 2000-10000 range might accomplish and what impacts there are of export operations that have almost always exceeded Vernalis flows in all but flood times. As the author points out on page 10, VAMP has not had the experimental conditions that would allow the impacts of different export rates at the same flow rate to be evaluated, nor have we had the higher range of VAMP conditions in any of the first six years. Nevertheless, the author plows ahead to resolve this issue, for which they have already acknowledged the absence of appropriate data. To address this use he uses multiple regression techniques on 7 data points. This is not the statistical approach behind the VAMP design and is inconsistent with the common guidance that one needs at least 10 data points for each variable entered into a multiple regression model. Not surprisingly, this attempt yields results that are difficult to interpret and which run counter to any hypotheses about the impact of exports on smolt survival. In footnote 19 the author recognizes that no consistent results come out of any permutations of this analysis. Despite this evidence that the data cannot support the analyses performed, the author combines the results with conclusions that adults returns reflect the importance of flood conditions on recruitment to argue that exports do not affect smolt survival. It is unfortunate that such unsupportable analyses were the basis for official recommendations from the State Department of Fish and Game on an extremely contentious issue amongst stakeholders and management agencies.

Comparable analyses would suggest that the Head of Old River Barrier must be bad for salmon because it only can be installed in drier years when production has always been low. In fact, this may reflect the thinking of the author, since the recommendations would prevent installation or use of a barrier at the head of Old River in all but dry and critical years.

Ocean Harvest. p. 15

The author evaluate the importance of ocean harvest on SJR salmon abundance by examining the correlation/regression between SJR escapement and the Central Valley Harvest Index. Despite finding a significant relationship, the author concludes that ocean harvest is not important and do not include it in their model. Given that adult abundance is one of only two factors included in the model it would seem to be important to include a factor which has been demonstrated to greatly affect adult abundance in neighboring stocks. Given that the Central Valley Harvest Index is our best guess of the impact of fishing on ocean stocks it seems an easy factor to include in the model. It is unclear what criteria the author used to include parameters in their model.

As its name implies, the Central Valley Harvest Index reflects the ratio of ocean harvest of all Central Valley stocks to the escapement of those stocks. As such, both the numerator and the

denominator in the harvest ratio are almost entirely driven by the year to year variability in Sacramento Valley escapement, which is on the order of 100,000 to 600,000 fish whereas the San Joaquin escapement varies from 227 fish to a maximum of 38,125. This difference in scale is so large that it is surprising that any significant correlation is found, as reported by the author. Two likely mechanisms probably explain this correlation between the harvest index and SJR escapement: 1. each year's SJR escapement is included in the harvest index so to some (small) extent the escapement is correlating with itself. 2. It seems more likely that the correlation arises from the simple straying of adults produced in the Sacramento River to spawn in the San Joaquin when Sacramento stocks are high. In any event, the correlation, or lack thereof, gives little insight into the importance of harvest on these stocks.

In-river adult salmon density p. 16

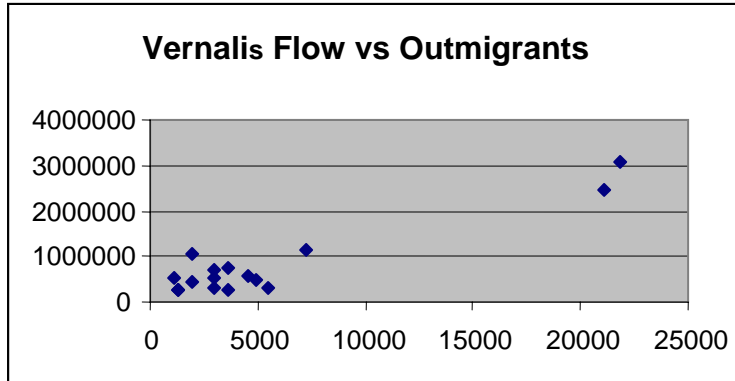
The author describes the three common patterns of recruitment – a straight line relationship where more spawners always increases the production of young, a Beverton-Holt curve where the number spawners reaches an asymptote in production and a Ricker model where increasing spawner density at some point results in actual decreases in production. The two latter density-dependent models have been shown appropriate for various salmon populations elsewhere; spawning habitat limitations can produce an asymptote whereas rearing habitat limitations are more likely to show a Ricker type of reaction. However, the author does not do a statistical test to determine which curve best fits the data but point to the fact that more spawners produce more fry as evidence that density dependence is not at work. This conclusion ignores that fact that all three models make that prediction. Visual inspection of the graph (figure 28) strongly suggests that a Beverton-Holt curve would be the best fit, which argues that density-dependence is at work in years of higher abundance; in fact the asymptote seems to be reached at female abundance on the Tuolumne River of about 4000 individuals. If this reflects spawning habitat limitation, than it points to a need to improve in-river habitat conditions.

Spring Flow p. 16-17

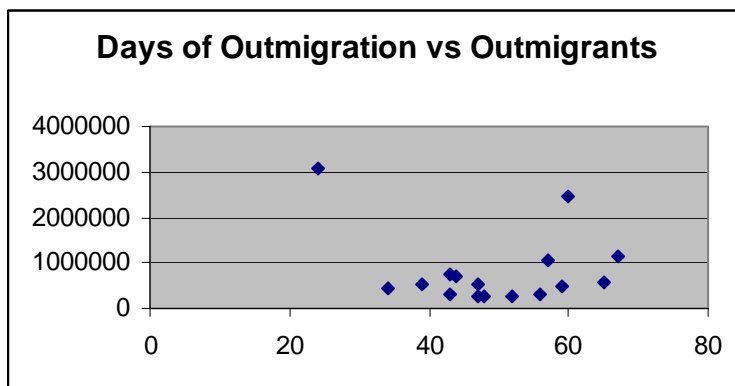
Despite their discussion on page 10 that the VAMP data are inadequate to evaluate the roles of flow vs export in delta survival, here the author concludes that “SJR salmon cohort abundance is strongly correlated with spring Vernalis flow magnitude and duration.” The author has already described how flows in the tributaries can significantly change temperature and spawning habitat conditions, but by focusing on cohort abundance only in relation to Vernalis flow the author disregards any ties to biological mechanisms. Mechanisms such as San Joaquin River restoration from Friant or recirculation of releases from the Mendota Pool have been proposed to increase flow at Vernalis, but those operations will not change conditions where the fish that are the focus of this report actually spawn, hatch, and rear. Spring flow at Vernalis may be important in assisting in smolt passage through the delta, but for all other life stages it is a poor surrogate for habitats that can be much better described directly.

Linearly connecting the historical conditions of flood conditions and dry conditions is the basis of the model. The two types of historical conditions are so different in almost all physical parameters that there is no reason to believe that linearly intermediate conditions will produce linearly intermediate results. Graphical examination of the data presented in Table 3

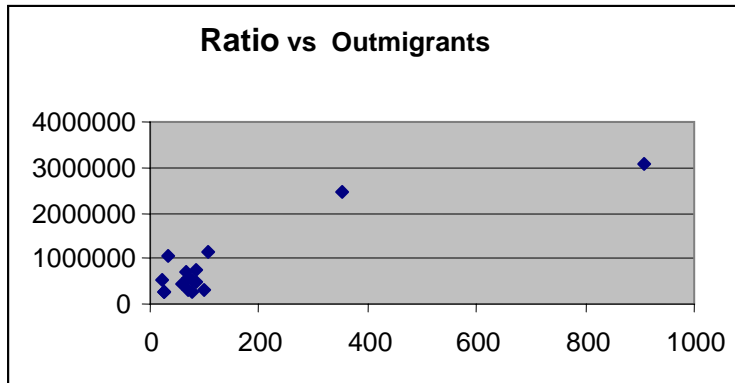
and Figure 32 (although there appear to be some discrepancies between the Table and the graph). The figure clearly shows that the regression is driven by two flood years (1995 and 1998) and that all other years show no apparent relationship to Vernalis flow. The graph clarifies that the recommendations for flows of 5000 cfs, 7000 cfs, 10000 cfs, 15000, and 20000 cfs do not reflect conditions for which much data is available.



The inclusion of 'duration' as a variable in the model is based on an unclear treatment of the ratio of days and flow that is not described in the document. From the data presented wet years showed both the longest (67 days) and the shortest duration (24 days) and critical years were almost as variable (34-57 days). The author seems to assume that the duration of the outmigrant period is the factor controlling subsequent return of the cohort. It is much more likely that years of low smolt abundance may appear to have a short emigration window because the sampling program can only detect fish at higher abundances – in years of high abundance the fish appear in the nets more regularly than in years when smolt are less abundant. It is not clear how the author developed their recommendations for number of days in the window of protection, but inspection of the data presented in Table 3 shows no relationship.



The author refers to a ratio of flows/days in support of their argument for including the number of days of outmigration as a regression variable. Such a ratio suggests that increasing number of days should lower the value of flow for outmigrants, but since the scale of flows is so much larger than the scale of days (1086 to 21808 cfs vs. 24 to 67 days) the resultant ratio is simply a restatement of the relationship with flow.



Specific:

Hydrology

1. Are the methods used in the Model (including Model Report) relating to flow sufficiently documented? If not, what improvements can be made to improve documentation?

The assumption that past relationships of flow with temperature and of Vernalis flow with production should be carefully considered. Other methods of increasing flow at Vernalis will likely not have the same effects and taking more water out of reservoirs than is done under present operating rules could easily change flow/temperature relationships. Temperature models are being developed and including them into the model would be much safer than assuming past relationships will hold.

2. What is the best metric (i.e. arithmetic mean, geometric mean, transformed data etc) that can be developed to adequately capture the variability in spring flow (i.e. magnitude and duration) on an intra-annual basis?

Limiting flows to be considered to flows that can be controlled would reduce a lot of this concern. The model suffers by trying to derive salmon management actions from flood situations. It is unlikely that flood conditions would be created for the benefit of salmon or that artificial flood conditions would have the same effects as historic floods.

3. What improvements to hydrologic data utilization can be made to enhance model prediction performance reliability?
4. Is there evidence of auto-correlation in flow calculations? If so, what is the affect? Does it need to be removed to improve model prediction (flow determination) reliability? If so, how can it be removed?

5. Are there additional flow metrics, parameters, logic etc. that should be incorporated into model logic and function? If so, what are they and how can they be assimilated into the model (reference to logic and function)?

Either limiting the scope of this model to the direct effects of Vernalis flows or expanding this model to incorporate all the factors affecting salmon escapement would be more useful than the attempt to disregard all other factors affecting salmon in favor of the one feature that reflects a complete state change of the system from flood control to water management.

Biology

1. Are the methods used in the Model (including Model Report) relating to fish abundance and/or production sufficiently documented? If not, what improvements can be made to improve documentation?

See above

2. What improvements to fish data utilization can be made to enhance model prediction performance reliability?

See above

4. Is there a way to improve how the model performs fish abundance prediction calculations and/or processing of fishery data?

See above

5. Is there evidence of auto-correlation in fish related calculations? If so, what is the affect? Does it need to be removed to improve model prediction (flow determination) reliability? If so, how can it be removed?

The absence of examination of stock-recruitment patterns in the model or the report is surprising. If SJR salmon are a self-perpetuating stock then we should see a stock-recruitment pattern and the model should not require inputs of the number of returning adults each year to keep it on track. If such auto-correlation is absent then attempting to apply a population model to the SJR salmon may be a vain undertaking. Recent genetic work certainly makes this a consideration that should be examined before any further population modeling work is done.

6. Does justification exist to include additional fish metrics, parameters, logic etc. in model logic and function (i.e. ocean harvest and/or Delta export entrainment)? If so, what are they and how can they be numerically assimilated into the model (reference to logic and function)?

Absolutely, see above.

7. How can model representation of hatchery production, and underlying model logic, be improved upon?

Test for impacts of hatchery on escapements. Include data from elsewhere about the survival of hatchery salmon in the field in comparison to wild stocks.

8. Currently the model predicts a constant ocean survival rate (i.e. relationship between cohort abundance and Chipps Island abundance is constant). Is there a need to make this relationship variable? If so, how can this be numerically accomplished in model performance?

The model should include CV hatchery index as an estimate of adult salmon loss (the author found a significant relationship in spite of considerable variance). Estimations of the impacts of ocean conditions could be included to estimate how often such conditions are drivers of adult abundance.

9. The model currently uses an adult replacement ratio of 1:1 as a numerically identified population health barometer. Is there a need to refine this ratio? What additional population parameter(s) could be incorporated into model logic and function?

Statistics

1. Currently the model uses liner relationships between flow and fish production because this relationship provides the strongest correlation value. Is it necessary to include a model toggle switch, model logic, and mathematical functions, that allow users the option to test a variety of non-linear relationships between flow and fish survival and/or production upon model results?

The two data points from high flow and the number of data points from low flow force the regression to effectively draw a line between two points. As the author implies, it is impossible from the data in hand to know if the line should be straight, or curved up or curved down without having data from intermediate Vernalis flows.

2. What is the statistical reliability of model out-put given that model predictions propagate? How can model reliability be improved?

For this sort of model I know of no way around the problem – stringing together variables must multiply the uncertainties. That can cast considerable doubt about the exact quantification of outputs but it does not mean that such models cannot usefully summarize our knowledge, identify the most important knowledge gaps and sources of variance. Tying together better strings of models – such as flow/habitat, flow temperature, spawning habitat/rearing habitat, velocity/transport, habitat/predation, export/entrainment, ocean

harvest/escapement, ocean temperature/growth&survival models allows for minimizing uncertainty at each step and knowing what one doesn't know. I cannot see how this model can do that – this model could become a delta survival model within that larger sequence, and that would highlight the need for delta survival data as the author states in the introduction.

3. Is collinearity present in model logic and/or computation, and what influence does it have upon model results? If present how can it be removed?

The model relies on collinearity to justify the use of a downstream variable to stand in for upstream conditions. Focusing the model on variables known to be important to particular life stages allows a parsing of effects that this model cannot.

4. In some cases, model predictions for salmon production occur outside the empirical data set range used to develop the regression. What limitations in model reliability result?

See above

5. Presently smolt survival has a statistically significant regression correlation with Delta inflow level (i.e. less than 7,000). No statistically significant regression correlation for juvenile smolt survival and Delta export level exists. However when inflow to export ratio is regressed against flow survival, a moderate regression correlation occurs. Currently, exports are not included as a model prediction parameter. Should exports be included as a model prediction parameter (for smolt production)?

See comments above about scope of model. Compared to flood years exports have little impact, but at less-than-flood conditions the question is unresolved and data need to be gathered at those mid-range flows – exactly as the author says.

6. Are the methods used in the Model (including Model Report) relating to statistical evaluations and/or model logic justification sufficiently documented? If not, what improvements can be made to improve documentation?

For comments on the statistical evaluation see above. DFG has recently brought in a high-powered statistician and his help here would doubtless greatly strengthen the discussion.

Including any reference to the published literature of salmon modeling would be welcome. Models exist both off the shelf and from related runs that could usefully be adopted or compared with SJR salmon models. The author seems to attach no value to work not done locally and that weakens the biological foundation and the resulting model.

7. What improvements to statistical use and application can be made to enhance model prediction performance reliability?

See above

8. There is substantial disagreement amongst scientists regarding the issue of density dependent mortality and its influence upon SJR salmon abundance (e.g. fall spawner abundance and spawning habitat availability: aka redd superimposition). In the absence of flow the relationship between spawner abundance and stock recruit appears to show density dependence (i.e. Beverton-Holt or other density dependent type relationship). However when flow is included with spawner abundance, in the form of a multiple-regression using spawner abundance and spring flow regressed against adult recruits, a significant correlation exists suggesting that density dependence does not explain the variation in SJR adult salmon escapement abundance. How can this issue be resolved with data to date, or in the future if data insufficiency exists currently?

See above:

In-river adult salmon density p. 16

The author describes the three common patterns of recruitment – a straight line relationship where more spawners always increases the production of young, a Beverton-Holt curve where the number spawners reaches an asymptote in production and a Ricker model where increasing spawner density at some point results in actual decreases in production. The two latter density-dependent models have been shown appropriate for various salmon populations elsewhere; spawning habitat limitations can produce an asymptote whereas rearing habitat limitations are more likely to show a Ricker type of reaction. However, the author does not do a statistical test to determine which curve best fits the data but point to the fact that more spawners produce more fry as evidence that density dependence is not at work. This conclusion ignores that fact that all three models make that prediction. Visual inspection of the graph (figure 28) strongly suggests that a Beverton-Holt curve would be the best fit, which argues that density-dependence is at work in years of higher abundance; in fact the asymptote seems to be reached at female abundance on the Tuolumne River of about 4000 individuals. If this reflects spawning habitat limitation, than it points to a need to improve in-river habitat conditions.

9. How can the statistical relationships between flow and fish survival and/or fish production be improved?

See above

Miscellaneous comments:

Salmon exhibit a complex life history and present considerable challenges for management. However, their importance to many people has led to a deeper understanding of salmon biology than we have for most species. On the San Joaquin River we are blessed with an abundance of studies that could provide an excellent basis for a population model that incorporated the knowledge and data that have been developed. The work of Steve Kramer on winter-run salmon shows that even simple spreadsheet models can usefully gather together the various factors affecting the diverse life stages of this complex species. Such a biological based model is particularly valuable in identifying the important data gaps that need to be filled to more effectively manage the species. This report describes a lot of the relevant

information but then discards most of it in favor of a regression model that captures almost none of the biology of this species and therefore provides no guidance to future research and with little relevance to comprehensive management.

In short, I find that most of the assumptions and conclusions are either not supported by the data or cannot be supported by the analyses. As a consequence I find the model to be unsuited for the purposes to which it has been put.

This report makes almost no reference to the mountain of work done on modeling salmon populations, in California and elsewhere. A quick Google search on “salmon ‘population modeling’” turns up 57,400 webpages and an abundance of technical resources. Almost the only bibliography items in this report are data sources and grey literature reports by this one office of DFG. In science one sees further by standing on the shoulders of those who have come before; this report would be substantially more valuable if it took advantage of the wealth of knowledge available on salmon population modeling.

Similarly, the report suggests that lower flows might be adequate if hatchery production was augmented. The author acknowledges that this is a ‘contentious issue’ but give no reference to the abundance of studies in the last 20 years that document the behavioral, genetic, disease, and environmental problems amplified by hatcheries and which have led to widespread abandonment of the use of hatcheries as mitigation for environmental degradation. Although the author states that he is not advocating for or against hatcheries, his only comments about them are the attribution of questionable benefits and the possibility that they could reduce the water costs of SJR salmon management. A more balanced discussion would be useful.